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A TAXONOMIST'S EXPERIENCE WITH HYBRIDS IN THE WILD[1]

By Dr. KARL M. WIEGAND

CORNELL UNIVERSITY

IT is with much trepidation that I approach this subject in an audience that is doubtless more deeply informed on the recent phases of genetics than I. The only justification for such audacity is that possibly some of the observations from another angle may supplement the splendid work of modern students in that now highly specialized field. I shall not attempt, however, a very deep genetical discussion of the observations made.

In his every-day experience with plants in the field the taxonomist is accustomed to think of the species in certain groups as clear cut and easy to work with, while other groups are difficult and the species more or less confused with no sharp boundaries. Much thought has been given at different times to the question as to why there is this difference between groups and what is its true phylogenetic significance. Long ago, among the special creationists there seemed to be no explanation other than that at the moment of creation the plans for these difficult groups had not been sufficiently worked out and perfected. Following the general acceptance of the theory of evolution, it seemed that in certain groups, species were perhaps in the making, through active variation, with the lines not clearly drawn as yet by natural selection. The more confused groups were therefore the newer groups. There still seems much reason for thinking this to be true in a general way.

That hybridity played any important part in causing the difficulty of species delineation in these groups was given little attention and rarely if ever suggested. The occurrence of hybrids in the wild was, in those days, thought to be a rather rare occurrence. In

[1] Address of the retiring vice-president and chairman of the Section for the Botanical Sciences, American Association for the Advancement of Science, Pittsburgh, December, 1934.

Gray's Manual (Edition 5), we find hybrids scarcely mentioned except in one genus, Quercus, where 5 were listed. In the 6th edition this had been increased to 9, and 9 hybrids in Carex were discussed. In this edition also mention was made of numerous hybrids in Salix. At that time there seems to have existed a rather indefinite prejudice against the hybrid. The hybrid tended to disorganize and do violence to the orderly classification of nature. So few were recognized, too, that the burden of proof was always with him who assumed the hybrid explanation. The matter was still more complicated by the lack of criteria for recognizing hybridity and to some extent also by the taxonomist's unfamiliarity in many cases with the behavior and characteristics of hybrids in cultivation. Some taxonomists apparently held the view-point that legitimate species should be difficult to cross, and therefore that wild hybrids should not be expected.

With the discovery of Mendel's studies on peas, the whole outlook toward hybridity began to change. These studies stimulated enormously an awakening interest in the problems of inheritance, and the whole modern science of genetics is the outcome. Along with this too was an awakened interest in the improvement of cultivated plants by breeding methods. One general result has been a more or less changed regard for the hybrid itself and it is no longer the outcast that it formerly was, but is full of interest as exhibiting some of the interesting and vital discoveries in heredity. In fact one author, Lotsy, has gone so far as to suggest the all-importance of the hybrid as the only source of new species in nature.

At about the time that Mendel's work was discovered by DeVries and others I became interested in the taxonomy of the genus Amelanchier, which presented forms in the region where I was then living that were at variance with the treatment of the genus in the different manuals. A wealth of forms was found there in central New York, but no satisfactory disposition of them could be made. Some years later, on removing to the vicinity of Boston, the same experience was repeated. Two summers spent in Newfoundland at about this time furnished a similar experience. In this latter country Amelanchier was abundant, but it seemed almost as though no two individuals were the same. A certain botanist who had given some attention to the genus stated to me that he seriously doubted whether species in the accepted sense existed in this genus at all. This general uncertainty induced me to seriously undertake a study of the genus to see if it were possible to discover the actual situation. Besides notes made in the field, more than a thousand herbarium specimens of East American Amelanchiers were assembled from different herbaria, and much time for a year was spent on the problem. Many sortings of this material were made, based on different structural characteristics, but at first without clarifying the situation.

The increasing interest in hybridity due to Mendel's work caused me at length to wonder whether crossing could have anything to do with the Amelanchier situation. So finally an attempt was made to sort the specimens as though there were a number of true species among them which when crossed with each other in pairs would account for the remaining forms. It was assumed that the true species when found would have certain characteristics, namely, they would probably each offer one or more characters peculiar to that species individually. Each species would also be expected to show a more or less normal range corresponding to the geographical regions of the country, as shown by the ranges of species in other groups. Also each would probably show certain soil and other habitat requirements, a certain period of flowering, and in other ways behave like species in other genera.

It was assumed also that hybrids would show certain peculiarities. They would not ordinarily present any new characters, but would simply recombine characters found in the two parent species, or in some cases show a blend of characters. The range too would tend to be small, as the hybrids presumably would not have had time to spread over wide areas, and thus their ranges might not follow the geographical areas, as possibly they had not spread that far. Also, hybrids would not be found outside the range of the two supposed parents.

Several sortings were made on this basis, but still with unsatisfactory results as far as the above points were concerned. Then suddenly a sorting was made that gave six piles of specimens. Each of these piles was essentially uniform and showed all the characteristics of a true taxonomic species as judged by the criteria just mentioned. In addition, there remained a seventh pile with quite different characteristics; it was not homogeneous as to character, it did not show new characters not present in the other six piles, and as far as distribution was concerned it was a mass of heterogeneous material. All the specimens in this pile, however, could be interpreted as crosses between some two of the other six piles.

We had then a rational disposition of the material, the first so far obtained. Here there were six good species similar to those in other groups, and the remaining material could be interpreted as hybrids of these species. The hybrid group, to be sure, was larger than might seem reasonable in nature, since it comprised about one third of the whole number of specimens. This, however, could be explained by the fact that while some of the six piles had been recognized before as common species and were familiar to

many collectors, all the hybrids were peculiar unidentifiable specimens and therefore were collected much more frequently than their comparative abundance in nature would lead one to expect.

Here we have an interpretation of hybridity based on what is really circumstantial evidence. Of course, the surest way to determine whether a plant is a hybrid is by breeding it to determine whether the characters behave in succeeding generations as hybrid characters should. I hear some of you ask why this has not been done. Most groups studied for revision by the taxonomist cover wide areas of country which, because of time and cost, he can visit only rarely and perhaps then not at the exact season for critical study of his group. He is forced, therefore, to depend to an undue extent on herbarium specimens. These obviously can not be bred, and indeed frequently the locality from which a particularly interesting plant came can not be visited. The only recourse is to depend upon circumstantial evidence of the sort just outlined. However, this does not mean, it seems to me, that such evidence and the conclusions drawn are necessarily of no value. In the few cases where breeding has been done, the original interpretations of the taxonomist have been generally supported. But if the plants can not be bred, it is still often very helpful to visit the locality from which a supposed hybrid was obtained. We may find not only the necessary parents but also other hybrid individuals showing other combinations of the parental characters. Some of these may be brought home and themselves bred. Also artificial hybrids between the supposed parents made in the garden will furnish material for comparison. Breeding, however, requires a certain special technique and space for growing the plants—requirements which render breeding by the taxonomist often very difficult. After all, in the study of large groups, chief dependence will still have to be placed on information gained from herbarium specimens. It has been suggested, to be sure, that hybrids may be told by the proportion of sterile pollen, and since that could probably be determined, at least roughly, in herbarium specimens, it might be an additional source of evidence. It is my belief, however, that investigations have tended to show this criterion to be of little use. Many apparently normal species often show as high as 50 per cent. sterile pollen (Erlanson on Rosa) while some hybrids have good pollen. Ecological conditions, also, appear to affect the viability of the pollen.[2]

Since the work on Amelanchier, the same problem has been met in the study of two groups of white asters. The same conditions were encountered, the same methods used, and the same conclusions reached as in the former case. In each of the groups of asters three or four species were found, together with a mass of specimens which could be reasonably accounted for only on the assumption that they were of hybrid origin with these species as parents. As in Amelanchier, the number of such hybrid specimens in the herbarium was greatly in excess of what one would expect from observations in the field, and the explanation is probably the same as in that case.

While these three studies have seemed to lead directly to the belief that hybridization in nature is a common phenomenon, other studies on other groups would seem to lead to the opposite conclusion. A revisionary study in two groups of Carex has shown no signs of hybridity. Neither were signs of hybridity found in the *Eupatorium purpureum* group, nor in Galium. Hybridization in the wild is therefore much more frequent in some groups than in others. It would seem that the Rosaceae in particular are liable to hybridize, and other taxonomically difficult genera in this family, such as Rubus, Rosa, Crataegus and others will probably be found to offer many cases of natural hybrids.

The query now arises: Are hybrids constantly produced in these groups, and, if not, what are the controlling factors? My experience tends definitely toward the conclusion that between certain species a great burst of crossing may occur locally all at once, and that this may or may not continue thereafter for a period of time. Lotsy in his field studies in South Africa has reached essentially the same conclusion. Over a limited area one will often find abundant hybrid individuals showing all manner of combinations of the characters of two parent species. The lay botanists get the impression that here the genus is "running wild." Elsewhere over large areas, even miles in extent, there may be no evidence of crossing whatever.

But what causes plants to hybridize thus locally and suddenly? My own observation seems to indicate that disturbance of the environment has something to do with the matter, and on this the following cases may throw some light. In Newfoundland a railway crosses the island. Near the railway the forest has been largely destroyed by fire or through logging operations. Tracts miles in extent are covered with dead brush and dense scattered thickets of scrub growth. Amelanchier is common in this region, but taxonomically it was found to be a mess, as almost every individual seemed different from its neighbor. In less disturbed areas the plants were more uniform. In the detailed study of this genus already alluded to, it appeared that many of the plants in the disturbed areas were hybrids, and the more uniform strains in

[2] See C. L. Huskins, SCIENCE, 69: 399, 1929.

the less disturbed regions were true species found generally in eastern North America.

Again, on the Blue Hills south of Boston, the forest has largely disappeared. Frequent fires have ravaged the region, and when visited several years ago presented alternating areas of bare rock, brush and open thicket. Amelanchiers were common, but few individuals were like any known species or like each other. Here also a peculiar micropetalous form occurred, previously noted by Dr. Robinson. Studies seemed to show that these aberrant forms were hybrids of three species occurring in the neighborhood, of which two were dry ground species and one an inhabitant of the swamps at the base of the hills. The micropetaly was due apparently to the effect of the fire, probably on the roots.

In eastern Maine near Pembroke Rubus was very abundant. In one locality the railway ran for a long distance through a lowland piece of woods. The forest came up to the right of way, which latter was kept cleared and mowed each season. Close to the track the ground was weeded frequently and therefore was very much disturbed. Along the border of the weeded zone prostrate, semi-prostrate and arching individuals of Rubus occurred in considerable numbers. These were "queer," many of them being unlike anything known to us. In the mowed zone there were fewer individuals and they were less queer, while over near the fence and bordering the woods the plants were, for the most part, clearly straight species with which we were familiar. Most, if not all, of these queer forms could be interpreted as crosses between the species near the woods, as they seemed to show only combinations of characters existing in these species. Not being a specialist in Rubus, I would hesitate to say that the aberrant forms were all hybrids, but they were very suggestive.

Another genus that has caused no end of trouble to the taxonomist is Crataegus. Here, in many parts of the eastern states, individuals of this genus occur often in very great numbers, forming extensive stands. The wealth of form is frequently very great, giving the impression at times that no two plants are alike. These individuals have been treated very differently by different taxonomists. Dr. Sargent, in his account of Crataegus in New York State, listed about 218 species, while Eggleston for the same area recognized only 38 species. Sargent, it is said, refused to believe that hybrids occur in Crataegus, or occur but rarely, while Eggleston is inclined to admit that hybrids are frequent or at times common. The species of Crataegus are chiefly pasture weeds in New York State, frequently destroyed by the farmer and subject to grazing by cattle. They can be said, therefore, to live under disturbed conditions. I suspect that the early botanist, before the country was settled, would have found no such wealth of form as exists to-day.

Hybrids in the field then tend to occur locally at some particular time and often in considerable abundance. While the cause of this is still obscure, a very suggestive hypothesis would connect it directly with the disturbed conditions just mentioned. It might well be that these conditions induce irregularities in the time of flowering. If some flowers are produced earlier or later than the normal period for that species, then these flowers might find no pollen from their own species but only that from some other species whose flowering period was earlier or later, as the case might be. Whether this is so has not been determined as yet. In this connection it is interesting that one author at least has cited dichogamy, especially in dioecious species, as a stimulus to hybridization. Obviously these suggestions here offered with regard to the time of flowering should not be given much weight until more evidence is at hand.

As to the significance in evolution and in the species question of this hybridization in the wild, much may be said, and much has been said. The literature of genetics contains many recent contributions to this subject, but unfortunately there are as yet very discordant views. At present there seem to be two generally accepted sources of divergence between parent and offspring, namely, gene mutation and hybridity. With the first we are not here concerned. In regard to the second, there have been very different viewpoints ranging from the extreme view of Lotsy, who saw no other cause for variation, to that of those who have wholly doubted the importance of hybridity in the production of new forms in nature. The old idea that hybrids are necessarily infertile is of course no longer held, as experiment has shown many cases of fertile hybrids. Reasons for fertility and infertility have been determined in many cases, and in plants the connection with polyploidy has recently been stressed. It is now known also that many hybrids may breed true, either as homozygous segregates or through connection with polyploidy. It is known too that in some genera, as for instance in the *Rosa canina* group, breeding true may occur through an apogamous development of embryos in seeds borne by the hybrid. But breeding true as recessives has been over-emphasized, it seems to me, as a cause of fixed races, as not all recessive characters are homozygous at the same time, and while one character has become pure many of the other innumerable characters going to make up the species concept are still heterozygous. Of course eventually one may get individuals having the composition a b c d e f g h i j, etc., but the chance is small. To be sure, if the plants were always

selfed the time required might not be so long, but few species are always selfed.

While fixed races can thus probably be produced by crossing, the question as to whether new species are formed in this way is another problem, and the evidence is not at all clear as yet. Are new characters thus created? Mendel in his experiments with peas obtained only recombinations of parental characters in the offsprings, and not new characters. In many cases it is now known that blends occur. To what extent they occur in nature is not yet clear. Casual observation does not easily discriminate between blends and the recombination of numerous closely related characters. Brainerd and Gershoy apparently found only blends in the F_1 generation of their violet hybrids, and often blends also in succeeding generations. While in a sense blends are new characters, they do not step outside the morphological range of the two parents, and they are doubtfully permanent. Where they occur they tend more toward a smoothing out of variation than toward wholly new forms. While geneticists have discovered some cases where new characters have appeared in hybrids, as for instance in Bateson's sweet peas, in Emerson's corn plant colors and in the walnut comb of fowls, I am not yet convinced that it is a sufficiently common occurrence to account, even in geologic time, for the great morphological diversity in plants. Many of the cases observed have had to do with color and not with structure, and are clearly due to the interaction of genes present in the parents rather than to new hereditary factors originating in the hybrids themselves.

Another serious difficulty in the way, if we are to consider hybridity as an important cause in the origin of species, is the dearth of evidence that it so operates in nature. I have already called attention to the situation in Amelanchier. After the hybrids were recognized and segregated there remained six or seven good species with normal ranges coinciding in general with the geographical areas in eastern North America, and fitting in with the ranges of other plants. The hybrids were usually local. Circumstantial evidence, therefore, seemed to indicate that these true species had been in existence a long time, during which they had spread over wide areas, as for instance from Newfoundland to Georgia and Minnesota—over all the area having suitable habitat conditions. They were old enough to have become more or less static as far as distribution was concerned. The hybrids seem like swarms of bees, buzzing around for a time, only to disappear, leaving the fundamental species to continue through the ages.

Professor Fernald has called attention to the fact that apparently in glaciated Nova Scotia only one weak species has been produced in the 25,000 or 30,000 years since the Wisconsin glaciation, and that in the glaciated coastal region of eastern North America *Bidens hyperborea*, occurring in several isolated localities, has maintained its specific identity during this same period. Though it belongs to a notoriously plastic genus, it has shown at the most only varietal deviations in these isolated habitats. In fact a geographer is led almost inevitably to feel that nearly all our east American plant species go back to the glacial period and probably far beyond. It can not be denied that species may have arisen through hybridity, quite possibly in the ways suggested by many geneticists, but one can not become very enthusiastic. At least, it seems evident that species are not being formed every day, or even every year, or even every century, as some enthusiasts are inclined to think.

What, then, becomes of the hybrids that from the standpoint of geological time are being produced in hordes? This to me is one of the interesting problems in evolution, second only to the problem of the origin of species in the first place. Geneticists have made suggestions in this connection. There would be, for instance, much sterility which would limit the number of offsprings. The hybrids would be free to cross again with the parent species in nature and homozygous recessives would be swamped. These recessives too would be weak in the struggle for existence. In these recessives, while they would be homozygous and recessive to one character, they would often be not so to others unless after a very long period of time and then only in comparatively few individuals. Most hybrids would tend to disappear therefore. The occasional production of stable polyploids would reduce this general tendency, but probably not to a marked degree. These genetical explanations of disappearance seem not quite sufficient to wholly account for the phenomenon, and we must await further studies.

One other problem remains in connection with wild hybrids. How shall we treat them taxonomically? Many systematic botanists have been loath to recognize them at all, or to admit that crossing occurs in nature. In recent years the belief in natural hybridization has greatly increased. A rational outlook, however, has been prejudiced by the unscientific attitude of some of these enthusiastic taxonomists. Many highly improbable and rash interpretations have been made that did not accord with the known facts. Since breeding is usually impossible and our decisions must be based largely on circumstantial evidence, special care should be used to see that our deductions are reasonable. The least that can be asked is that these deductions should be based only on careful and painstaking analysis of the evidence. Some recent writers have even assumed crossing between parents that do not grow in the locality or even in that part of the country. Often general impressions and not an analysis of characters have been sufficient to impress these

enthusiasts. The only result is to discredit taxonomy.

But equally unfortunate is the attitude that would prohibit the recognition of natural hybridization except in hybrids actually bred genetically. The argument that a supposed hybrid, unbred, is merely a matter of personal opinion, and therefore should not be recognized, is invalid, for so are species, genera and families matters of personal opinion. In any taxonomic manual the groups there presented represent the author's interpretation of nature and nothing more. These groups are based likewise on circumstantial evidence, since he has not seen these species arise in nature, and represent only the conclusions reached from careful study of existing material. Why, then, should the taxonomist be adverse to the recognition of hybrids, who is perfectly willing to accept innumerable new species on much less reasonable ground? Taxonomists who do not recognize hybrids are often forced to treat such suspected forms as species. They are willing to assume them to be species until some one proves them to be something else, thus placing the burden of proof on the other fellow. I, personally, belong to that benighted group of taxonomists, who believe that new species should not be proposed as such until the author can not reach any other conclusion. Is it not our duty to science, to workers in other fields and to our fellow taxonomists not to clutter up our subject with endless names and half-baked concepts which seem only to confuse and to cause resentment and to pass the buck? The science of taxonomy stands too low now in the estimation of general workers.

How, then, should hybrids be named? The recognition of supposed hybrids as hybrids should not necessarily increase the number of names which we all deplore. It is my preference to designate ordinarily such a plant by the expression *Quercus bicolor* × *macrocarpa* and not by a new name. The only exceptions would be a few hybrids that in horticulture have acquired well-known specific names. Into this condensed designation *"Quercus bicolor* × *macrocarpa"* we would always read the expression, "A probable hybrid of *Quercus bicolor* and *Q. macrocarpa,* according to my interpretation" unless the hybrid had been actually demonstrated by breeding or synthesis. But so do we also read into the designation *"Q. macrocarpa* Michx." as a species, the statement, "A species, *Q. macrocarpa* in the sense of Michaux as interpreted by me." I can not help but see at least a practical difference between the causal more or less evanescent and temporary hybrid and the fundamental established species reaching back perhaps to the glacial epoch or beyond. The six fundamental species in the Amelanchier study did not seem in the same category with the hybrids, which, when eliminated, revealed them. I prefer to restrict the binomial to these older fundamental units for clearness and also on sentimental grounds.

In conclusion it may be asked again what relation then this experience with hybrids in the wild bears to the problem of the origin of species. As already mentioned, Lotsy was of the belief that hybridity is very likely the sole cause of the origin of new forms. Most biologists, I believe, are not ready to take a stand so extreme, but many students of genetics feel that the stable hybrids produced in their experiments represent at least one way by which new species may arise. While I would not really question this last statement, the point interesting to me is the almost[3] total lack of support for this view in our experience with the wild hybrids. As pointed out, the mass of hybrids in the cases under observation seem wholly casual and in no way to affect the fundamental species, which presumably have existed almost unchanged since the glacial period or before. It is still possible that these fundamental species came about by hybridity, and that others will also, if sufficient geological time is allowed. It would seem, however, that the factors noted by the geneticist ought to produce stable forms much sooner than that, even at the longest. Still another question, of course, is whether hybridity, which combines the genes of two parents, could produce new characters often enough and of sufficient magnitude to account for the great morphological diversity in plants. It is clear, I think, that we have not yet solved the problem of the origin of species.

The observations and view-points expressed in this paper are of course those of one person only, and are presented for what they may be worth. However, the angle from which they are presented is not quite the conventional one, and this may be an excuse for afflicting you with them.

OBITUARY

ROLAND BURRAGE DIXON

ROLAND BURRAGE DIXON, the senior member of the division of anthropology of Harvard University, died on December 19, 1934. He was the greatest ethnographer whom this country has produced. Dixon was born at Worcester, Massachusetts, on November 6, 1875. He took his A.B. degree at Harvard in 1897 and his Ph.D. in anthropology in 1900. From the year of his first degree until his death he was continuously

[3] A few cases: Huskins (*Genetics,* 12: 531, 1931) claims a hybrid origin for *Spartina Townsendii* and Müntzing (*Hereditas,* 14: 153, 1930) describes a hybrid in his cultures indistinguishable from *Galeopsis Tetrahit* L. both morphologically and in chromosome number—a synthetic *G. Tetrahit.* Should be further studied.

in the service of Harvard University, passing through the various academic grades until he was made full professor of anthropology in 1915. In 1904 he became librarian of the Peabody Museum, in 1908 secretary and in 1912 curator of ethnology. At the time of his death he held all these positions.

In the earlier years of his professional career, Dixon did extensive field work in anthropology. He carried on archeological excavations in Ohio, made ethnological researches among the Indians of British Columbia and Alaska, and spent no less than six seasons of work among the California Indians. His subsequent travel and investigation took him to New Zealand, Tasmania, Australia, Fiji and various parts of Asia. Nevertheless, Professor Dixon was primarily a student of anthropological literature rather than a field worker. He acquired a reading knowledge of numerous foreign languages, which, with his indefatigable industry, enabled him to master existing knowledge of the anthropology of four great continental areas: North and South America, Oceania and Asia. He classified and digested this prodigious mass of information, put it in card catalogues, and made it the basis of ethnographic lecture courses which were a model of organization and were exhaustive yet stimulating. From no other anthropologist in the world could students acquire a similar mastery of anthropological fact. Dixon carried an incredible store of this knowledge in his head and could produce instantaneously a detailed and sometimes complete bibliography of any subject within his chosen areas. He even succeeded in keeping up to date with the literature of his subject, and, so far as possible, read all of it.

In the Peabody Museum library Dixon established a catalogue system whereby books and articles in anthropological periodicals were classified not only by author but also by subject and by area. His unflagging energy in pushing forward this formidable task has made the anthropological library of the Peabody Museum the most easily utilizable and the best organized for research of any collection in the world.

Dixon's particular anthropological interest was in material culture and its diffusion. He wrote many articles on this subject—all notable because of his scholarship and his refusal to be lured from the path of scientific truth by romantic theories. His larger works, apart from technical monographs, include a book on the mythology of Oceania, a volume entitled "The Building of Cultures" and his widely discussed "Racial History of Mankind." The last-named was an adventurous foray into the field of physical anthropology, whereby the peoples of the world were classified according to the tripartite categories of three cranial indices as combined in individuals. This work was based upon a complete study of existing anthropometric material, and was a pioneer effort to establish the principle that racial classification should be based upon individual combinations rather than upon isolated group means. In spite of the vulnerability of Dixon's method in several of its processes, he succeeded in establishing a considerable number of important new points concerning human distribution and migration. Many of these have been confirmed subsequently by independent investigations of other scholars employing more elaborate methods than his widely condemned "short cut." Dixon was accustomed to refer to this book jocosely as "my crime," but, in the opinion of the present writer (who disagrees profoundly with many of Dixon's results and with most of his methods), "The Racial History of Mankind" is the most provocative and brilliant book of his anthropological generation. It will be perused when many safe and sane anthropological works have been forgotten.

Dixon was a solitary bachelor who lived contentedly in a beautiful country home, intentionally selected for its remoteness from Cambridge. Three times a week he emerged from his seclusion to empty upon his students his capacious vials of knowledge. Upon graduate students, engaged in research, he lavished his time and his inexhaustible supply of knowledge. As a director and critic of research Dixon was superb. In examinations he was formidably exacting, unsympathetic, but just. He commanded the fear, admiration and respect of his students, and the complete confidence of his colleagues. He labored incessantly and effectively to develop at Harvard a well-rounded anthropological curriculum based upon sound and conservative scholarship and thorough factual knowledge.

Professor Dixon was encrusted with an almost impenetrable reserve, topped with a high gloss of genial courtesy. Almost no one had access to the arcana of his personality. He was, underneath, a sensitive and kindly man, who led his life according to his own private rules and measured up to his own lofty ideals of conduct and performance. Throughout a protracted and wasting illness, he fought indomitably and stubbornly to continue in the discharge of his duties, never admitting to his colleagues (if indeed to himself) the inevitability of his defeat. He fully merited the Horatian encomium, "iustum et tenacem propositi virum."

E. A. HOOTON

RECENT DEATHS

DR. DAVID WHITE, senior geologist in the U. S. Geological Survey and recipient of the Wolcott award from the National Academy of Sciences, died on February 7 at the age of seventy-two years.

ROBERT R. ROWLEY, instructor in science at the Louisiana high school in Louisiana, Mo., and formerly paleontologist with the Missouri Geological Survey, died on January 26 at the age of eighty-one years.

DR. ROGER H. DENNETT, professor in children's diseases at the New York Post-Graduate Medical School and director of the pediatrics department of the Post-Graduate Hospital, died on February 3 at the age of fifty-eight years.

FREDERICK O. WILLHOFFT, formerly professor of mechanical engineering at Columbia University, died on February 6 at the age of fifty-eight years.

HARRY DE BERKELEY PARSONS, professor emeritus of practical engineering at the Rensselaer Polytechnic Institute, died on January 26. He was seventy-three years of age.

DR. EDMUND B. PIPER, professor of obstetrics at the University of Pennsylvania Medical School and Graduate School of Medicine, died on January 14. He was in his fifty-fourth year.

SCIENTIFIC EVENTS

ACQUISITIONS OF THE BRITISH NATURAL HISTORY MUSEUM

THE London *Times* reports that among recent acquisitions of the Natural History Museum, South Kensington, is an important collection of 300 birds obtained by A. W. Vincent in the southeastern district of the Belgian Congo. This area has been very little investigated from the ornithological point of view, and the accession includes many forms hitherto unrepresented or very poorly represented in the national collection.

A series of skulls of the larger Indian carnivores has been presented by Lieutenant-Colonel J. H. Carlisle, and a collection of game trophies from Northern India and Upper Burma by Colonel C. E. Nichol. Miss A. E. Thomson has given a very rare flying squirrel from Borneo.

A valuable addition to the entomological department's collection of *Hemiptera* consists of 17 specimens of *Termitaphidae,* presented by Dr. J. G. Myers, of the Imperial College of Tropical Agriculture, Trinidad. These rare and little-known insects are found only in the nests of white ants in America and the Old World, but the nature of the association is not known. Superficially they suggest in appearance diminutive woodlice or scale insects.

A purchase of particular interest is a collection of 500 beetles from Tibet, Central Asia, Western China and the Altai Mountains, the majority of which are paratypes of species hitherto unrepresented in the department. The Public Schools Exploration Society has presented the whole of the entomological collections made during their recent expedition to Newfoundland.

Geological acquisitions include 100 specimens of primitive fish-like Ostracoderms, obtained by Wickham King, chiefly from the old red sandstone of Worcestershire; and a fine series collected by Dr. E. I. White, and H. A. Toombs from Herefordshire, comprising many forms new to science.

As a bequest from the late T. B. Clarke-Thornhill, the mineralogical department has acquired a valuable collection of gemstones; the 90 cut stones, many of them of large size, include 16 fine colored diamonds, parti-colored corundum, tourmaline, opal, alexandrite, etc.; and there are uncut specimens of opal and moonstone and large masses of Kaurigum from New Zealand.

The first meteorite to be recorded from Rhodesia, a stone weighing 481 pounds and 11 ounces, which fell last March in the Mangwendi native reserve, 40 miles east of Salisbury, has been presented by the government of Southern Rhodesia.

A NUTRITIONAL STUDY OF BELGIAN UNEMPLOYED

ACCORDING to the *British Medical Journal,* an investigation into the living conditions and budgets of insured unemployed in Brussels was carried out in 1932, and the sociological results have already been published. Bigwood and Roost now record the nutritional data under the title "L'Alimentation Rationnelle." The facts were obtained from a month's study (January to February) of nineteen families, chosen at random from the lists of unemployed, which comprised ninety-three persons. Quantities of foodstuffs bought or given were entered in notebooks. The analyses of foodstuffs used for computation were chiefly those of Van de Weyer for Belgian produce, with special analyses where necessary. Foodstuffs as consumed probably did not vary more than from −3 to +3 per cent. from analytical tables. Refuse ranged from 7.5 to 14 per cent. of total foodstuffs as bought (average 11 per cent.), high percentages being obtained chiefly where the amount of potatoes was large. Plate-waste was calculated as 1 per cent., intestinal waste as 4 per cent.; protein and carbohydrate were calculated to yield four calories per gram, fat nine calories.

Complete tables are given for each family for gross and net calories, grams of animal and vegetable protein, fat and carbohydrate. The percentage amount

of the total gross weight supplied by the various food groups averaged as follows: meat 9; cereals, etc., 55; vegetables 6; fruit 2; fats 3; eggs 1; milk 18; cheese 0; various 6. Family coefficients according to different scales are compared, and that adopted takes the woman as unit and allows her 2,600 calories net, the unemployed man being allowed 2,400, or 0.90, and children scaled down according to age to 0.25 for a child under 1 year. The authors reckon that the League of Nations scale of calorie requirement is slightly below that of the Belgian people, as shown by Slosse's investigation into 1,065 working men in 1910.

The net calories of the nineteen families varied from −33 to +30 per cent. on the Bigwood-Roost scale, five families being more than 10 per cent. below and therefore certainly receiving insufficient food. Protein averaged 81.5 grams per unit, with a range of 55 grams to 105 grams, of which the animal protein averaged 40 per cent. (range 27 to 52 per cent.). For each gram of protein the average intake of fat was 1.1 grams and of carbohydrate 4.6 grams. The minerals per unit were: Phosphorus, 1.44 grams; calcium, 0.74 gram; iron, 0.015 gram; calcium-phosphorus ratio, 1:1.95; calcium-protein ratio, 1:118. Of the vitamins the B complex was probably sufficient, A or D approximately half of standard requirements, C rather more than half, after allowing for loss in cooking. It must be borne in mind that the number of families studied is too small to allow of general conclusions being drawn. This study rather suffers from lack of sequence in arrangement and of clarity in the graphs. There are nearly a hundred tables, many of which need not have been included, while the addition of more tables summarizing the figures would be of great advantage.

PROPOSED STATE FORESTS IN MASSACHUSETTS

A TENTATIVE plan has been prepared by Commissioner Samuel A. York, of the Massachusetts State Department of Conservation, for an orderly establishment of state forests and parks, making use of idle land which at present is of little if any value to either the owners or the communities. Mr. York, according to the *Boston Evening Transcript,* explained to representatives of the Governor's Committee on the Needs and Uses of Open Spaces that the plan shows in a general way where the land is available for purchase by the state, at a cost of about $5 per acre, which is all the state can pay under the present law, and if the plan is carried out there will be public reservations for recreational purposes within fifteen miles of every large center of population. Charles Sumner Bird, Jr., chairman of the committee, presided over the conference.

The program would be to buy 30,000 acres a year for ten years, and to acquire for the public six ocean beaches within five years. Salisbury Beach has already been acquired, and is now under the management of the conservation department.

Commissioner York gave six specific reasons for the plan. They are to reforest the waste land in the state; to provide healthful outdoor recreation for the public; to provide worth-while work that is non-competitive with private industry; to bring increased revenue from tourists; to stabilize rural employment and to preserve and increase the annual $200,000,000 recreation industry of Massachusetts.

Each of the areas is to be at least 5,000 acres in extent and is to be developed for recreation, wild life and forestry. The land needed can be bought for $5 an acre, and for a time at least the development will be made by the use of relief funds. Relief funds can be used that way to better advantage than if passed out as a dole. If the plan were followed, creating about twenty-six areas in the state, the total acreage would be in the vicinity of 740,000 acres inclusive of what the state already owns, as in many instances the new purchases would be to enlarge present state forests.

As to the cost of the project Mr. York explained that much of it would be self-supporting, because the policy will be followed of charging for the special services given. The public will have free access to parks, forests and water fronts, but there will be cabins, fireplaces supplied with wood and other facilities for which it will be deemed proper to charge the users a fee. And after some years the forest growths will have developed to the point that lumber may be cut and sold.

COMMITTEE ON UNEMPLOYMENT AND RELIEF FOR CHEMISTS AND CHEMICAL ENGINEERS

THE Committee on Unemployment and Relief for Chemists and Chemical Engineers, of which A. Cressy Morrison is chairman of the finance committee, has sent out the following appeal:

> Your committee has periodically brought to your attention its efforts to alleviate distress in the profession.
>
> During the last three years, more than 1,400 qualified chemists and chemical engineers approached the committee for help. More than 600 were placed on permanent or temporary jobs. Financial assistance was secured for 167 families in desperate need.
>
> Nearly 1,000 chemists and chemical engineers have, to date, contributed a total sum of about $35,000—or an average of $1.00 per month per contributor. Our present

funds will soon be exhausted. With this inadequate sum, your committee was able to carry on because of the strictest economy, free office space and furniture, voluntary services, and help from government welfare organizations. Due to these agencies, a large number of very serious cases were placed on temporary relief projects. Thanks to all our contributors, no case of hunger or inadequate clothing has left our doors without immediate relief and encouragement.

With the approaching winter and continued unemployment, additional funds must be secured to help 150 whose resources are exhausted. The reserves of an additional number of your brother chemists are nearly depleted, and a total of 426 on the committee's active files need jobs very badly. The committee, therefore, earnestly asks your serious consideration of this problem and urges your immediate support.

Our records show that you have not contributed to the committee so far, but we hope that your present circumstances, as one still employed, will enable you to do your part to help those of your professional associates in need.

Your cooperation in reporting jobs to the committee's office will be deeply appreciated. If you know of any one unemployed, ask him to register. Bring the committee's efforts to the attention of your coworkers.

Checks should be made payable to Robert T. Baldwin, treasurer, and should be sent to 300 Madison Avenue, Room 1004, New York City.

ANTHROPOLOGISTS AND THE FEDERAL INDIAN PROGRAM

ANTHROPOLOGISTS from all parts of the United States, at their meeting at Pittsburgh, Pennsylvania, December 29, pledged assistance to Commissioner Collier in the work of rehabilitating Indian communities and developing an Indian program directly related to the life and needs of Indian people.

Commissioner Collier's address on the new Indian program came at the conclusion of the three-day sessions of Section H of the American Association for the Advancement of Science. In his address, Commissioner Collier emphasized the land acquisition policy, developments under the Indian reorganization act, and especially the participation of Indians in the new plans. A vigorous discussion followed the commissioner's presentation, in which enthusiastic approval was manifested in regard to many of the efforts that are being put forth to aid Indians in achieving economic, social and cultural opportunities.

Saturday evening was given over to a special Indian Service conference in which an invited group of anthropologists discussed with Commissioner Collier and members of the Indian Service staff such questions as: (1) In what way can we most effectively utilize contributions of anthropology in the work of organizing Indian communities under the Indian Reorganization Act? (2) What types of training shall we set up for both Indians and whites already in the service and for those who may come in later? (3) What shall be the form of continued coperation between the Indian Service and the anthropologists? Secretary Wallace of the U. S. Department of Agriculture was the first speaker at this evening conference and presented vigorously and sympathetically his views of the economic and cultural needs and possibilities of the Indians.

Members of the conference suggested a census of anthropologists as a means of putting at the disposal of the Indian Service anthropological workers acquainted with specific areas. There was also general agreement as to the need for assigning a consulting anthropologist to the staff of the Indian Office, whose duty it would be to make necessary contacts with anthropological groups throughout the United States. Regional conferences of anthropologists and others were also recommended.

In a memorandum presented to the anthropologists in advance of the meeting as a basis for discussion, certain of the projects involving anthropology already under way were presented, including the kind of constitutions for Indian communities and the provisions made so far for Indian Service workers trained in anthropology and related fields.

Among the anthropologists who accepted the invitation to be present were Dr. Fay-Cooper Cole, University of Chicago; Dr. Radcliffe-Brown, University of Chicago; Professor E. Sapir, Yale University; Dr. Duncan Strong, Bureau of American Ethnology; Father John M. Cooper, of Catholic University, secretary of the American Anthropological Association; Professor A. E. Jenks, University of Minnesota; Professor Ralph Linton, University of Wisconsin; Dr. George Herzog, Institute of Human Relations, Yale University; Rev. Berard Haile, Gallup, New Mexico; Professor Robert Redfield, University of Chicago; Dr. Margaret Mead, Museum of Natural History, New York City; Dr. Herbert J. Spinden, Brooklyn Museum; Dr. Alexander Lesser, Columbia University; Professor Leslie A. White, University of Michigan; Mrs. Elsie Clews Parsons, Harrison, New York; Dr. Melville J. Herskovits, Northwestern University; Professor Leslie Spier, Yale University; Dr. Diamond Jenness, National Museum of Canada.

In addition to Commissioner Collier, the following represented the Indian Service and Interior Department at the meeting: A. C. Cooley, director of extension; Felix Cohen, of the solicitor's office; Miss Mary McGair, assistant to the commissioner; Samuel M. Dodd, budget officer of the Indian Service; F. H. Dailker, junior assistant to the commissioner; Miss Elinor D. Gregg, supervisor of nurses; Miss Sally Lucas Jean, administrative coordinator in the Navajo Area and supervisor of health education; Miss Edna

Groves, supervisor of home economics; Miss Rose K. Brandt, supervisor of elementary education; Miss Mary Stewart, assistant director of education; Mrs. Elna Smith, of the division of subsistence homesteads; Miss Evelyn Pierce, of the education division; Mrs. Margaret Welpley, of the Indian organization unit, and Albert Sandoval, of Lukachukai, Arizona, assistant on the Navajo language project.

SCIENTIFIC NOTES AND NEWS

DR. CHARLES A. KRAUS, professor of chemistry and director of chemical research at Brown University, has been awarded the Willard Gibbs Medal of the Chicago section of the American Chemical Society for 1935, for his research on the theory of solutions.

PROFESSOR CARL G. ROSSBY, professor of meteorology, and Dr. Hurd C. Willett, assistant professor of meteorology at the Massachusetts Institute of Technology, on January 30 received the Sylvanus Albert Reed award of the Institute of the Aeronautical Sciences in New York for their contributions to weather forecasting. It was largely as a result of their work that the polar front or air mass analysis method recently was adopted by the U. S. Weather Bureau. Dr. Joseph S. Ames, president of The Johns Hopkins University and chairman of the National Advisory Committee for Aeronautics, was made an honorary fellow of the institute.

THE Stevens Triennial Prize was awarded in 1934 by the College of Physicians and Surgeons of Columbia University to Dr. Robert F. Loeb and Dr. Harold A. Abramson, jointly. The award to Dr. Abramson was made on the basis of his research on the electrochemical reactions of blood cells and is of interest because of his recent summary of the electrochemical properties of surfaces in liquids.

AT a recent meeting of the American Society of Civil Engineers the Norman Medal was presented to Leon S. Moisseiff, consulting engineer of New York City, for his paper on "The George Washington Bridge—Design of the Towers." The J. James R. Croes Medal was awarded to Dr. H. M. Westergaard, professor of theoretical and applied mechanics at the University of Illinois, for his work on "Water Pressure on Dams during Earthquakes." The Thomas Fitch Rowland Prize was awarded to Miles I. Killmer for a paper on "Fulton Street, East River, Tunnels." A paper by E. Warden Bowman on the steel superstructure of the George Washington Bridge won the James Laurie Prize. J. C. Evans, terminal engineer of the Port of New York, received the Arthur M. Wellington Prize. Mr. Evans's paper dealt with the approaches and highway connections to the George Washington Bridge. The Collingwood Prize for juniors was given to G. H. Hickox and G. O. Wessenauer, for a paper on the "Application of Duration Curves to Hydro-Electric Studies."

AT the dinner of the American Society of Plant Physiologists in Pittsburgh, Dr. Burton E. Livingston, president of the society, was presented with a check by his former students at the Johns Hopkins University, on the occasion of his completing twenty-five years of service at this institution and in recognition of his services to plant physiology. The presentation was made by Dr. John W. Shive, professor of plant physiology at Rutgers University, who received his doctor's degree from the Johns Hopkins University in 1915.

DR. LEONA BAUMGARTNER, of Yale University, now of the Children's Clinic of the New York Hospital, was awarded the John Lovett Morse Prize for her research work upon "Age and Antibody Production." The prize is given annually by the New England Pediatric Society for the outstanding investigation of the year.

DR. EDNA HARDE YOUNG, American bacteriologist connected with the Pasteur Institute, has been awarded the Guy Amerongen Prize for cancer research, for a paper submitted to the French League against Cancer on the thesis that chemically preserved foods aid the growth of cancer cells.

IGNATZ MOSCICKI, president of Poland and well known for his work in chemistry, was awarded the degree of "doktora honoris causa" at the University of Fribourg in recognition of his thirty years of research in electrochemical research.

HARVARD UNIVERSITY has announced the following retirements and appointments: Dr. George H. Parker, professor of zoology; Dr. Milton J. Rosenau, professor of medicine and hygiene, and Albert Sauveur, professor of metallurgy and metallography, have retired. Dr. Weld Arnold, instructor in geological survey and field astronomy, has been appointed assistant director of the Institute of Geographical Exploration for one year; Edwin Raisz has been made curator of maps in the Institute of Geography, and Edmund B. Delabarre, Jr., has been made assistant in psychology.

DR. KNIGHT DUNLAP, professor of psychology in the Johns Hopkins University, has accepted appointment as visiting professor at the University of California at Los Angeles for the spring semester.

IT is reported in *Industrial and Engineering Chemistry* that Dr. Irvin Lavine, professor of chemical

engineering at the University of North Dakota, has been appointed by the National Resources Board of Washington, D. C., to the position of consultant to the North Dakota State Planning Board. He will work with the state board in developing a long-range program for the rehabilitation of the state according to objectives laid down by the National Resources Board. The university is extending a leave of absence to him for the period of his appointment. His headquarters will be at the university.

DR. THOMAS A. WILSON, associate professor of chemistry at Union College, has been appointed senior fellow at the Mellon Institute. Dr. J. George Lutz has been made instructor to replace Dr. Wilson.

DR. ALBERT C. FURSTENBERG, professor and head of the department of otolaryngology, has been appointed dean of the Medical School of the University of Michigan.

HUMPHREY GRAY OWEN, associate professor of zoology at the University of Denver, has been made chairman of the division of biological sciences and professor of zoology. He succeeds Dr. Ira Eugene Cutler, who becomes professor emeritus.

THE resignation of Claude R. Kellogg, assistant professor of entomology at the Massachusetts State College, has been announced. Professor Kellogg left on February 1 for China, where he will resume the post of professor of entomology and beekeeping at the Fukien Christian University which he held from 1916 to 1931.

E. F. ARMSTRONG, British industrial chemist, has been elected chairman of the British Standards Institution.

DR. WALTER B. CANNON, George Higginson professor of physiology at the Harvard Medical School, will leave early in March for China, where he will serve as visiting professor of physiology at the Peiping Union Medical College from April 15 to June 1. After a sojourn in Japan, he will cross Siberia and attend the International Physiological Congress in Leningrad and Moscow in August.

J. FRANCIS MACBRIDE, assistant curator of taxonomy in the department of botany of the Field Museum, who has been in charge of the joint botanical project of the Rockefeller Foundation and the Field Museum of Natural History since it was inaugurated about five years ago, sailed for Europe on January 30 to continue the project. Mr. Macbride has been in this country for a visit during the past several months, his first interruption of the work since its inception. The project has for its purpose the making of photographic negatives of type specimens of plants preserved in European herbaria, prints of which, through the Field Museum, are made available to all botanists.

DR. HELLMUT DE TERRA, research associate at the Peabody Museum, Yale University, and chief of the Yale University North India Expedition, sailed on February 2 for India to take charge of excavations in the Himalayan foothills. The expedition will seek further traces of prehistoric man to match with the fossil ape unearthed in Northern India last year. The expedition, sponsored by Yale University, the Carnegie Institution of Washington, the American Philosophical Society, the Geological Survey of India and the Cenozoic Research Laboratory of Peking, China, will have an international personnel. Père Teilhard de Chardin, acting director of the Cenozoic Laboratory; T. T. Patterson, a geologist and archeologist of Cambridge University, and V. N. Ayengar, paleontologist of the Survey of India, are members of the party.

A SERIES of lectures has been inaugurated at the School of Medicine and Dentistry of the University of Rochester, to be known as the Eastman memorial lectures, in memory of George Eastman. The last lecture was given on November 14 by Dr. Eugene F. DuBois on "The Surface Area of the Body and the Radiation of Heat." On February 5 Dr. Peyton Rous lectured on "Viruses and Tumors."

DR. W. F. G. SWANN, director of the Bartol Research Foundation, lectured at the first meeting of the Lancaster, Pennsylvania, Branch of the American Association for the Advancement of Science on February 13. His subject was "Cosmic Rays."

PROFESSOR ALBERT EINSTEIN was the principal speaker at a dinner given at the Young Men's and Young Women's Hebrew Association in Philadelphia on February 6, as the opening of a campaign to raise $25,000 for the support of the Bicur Cholim Hospital in Jerusalem.

DR. ARTHUR L. DAY, director of the geophysical laboratory of the Carnegie Institution, delivered an address before the Washington Academy of Sciences on January 17. The title of his address was "Public Safety in Earthquake Regions."

THE annual Alpha Omega Alpha lecture of the Jefferson Medical College was given on January 18 by Professor William K. Gregory, of the American Museum of Natural History, on "The Origin, Rise and Decline of *Homo sapiens*."

A SERIES of lectures on our astronomical relations was given in January at Harvard University by Dr. Harlan T. Stetson.

DR. RALPH H. MAJOR, professor of medicine at the University of Kansas School of Medicine, gave the annual Scripps lectures at the Scripps Metabolic Clinic at La Jolla on November 11, 12 and 13.

DR. GEORGE GAMOW, visiting professor at George Washington University and research worker in atomic nuclei at the Academy of Science of Leningrad and at the University of Copenhagen, delivered two lectures at the University of Pennsylvania on January 31. A talk on "Modern Alchemy" was given by Dr. Gamow in the afternoon and in the evening he lectured on "Elementary Particles in Physics."

DR. H. D. CROCKFORD, associate professor of physical chemistry at the University of North Carolina, has received a grant of money from the U. S. Naval Research Laboratory of the Navy Department in Washington, to direct research on the thermodynamics of the lead-sulfuric acid storage cell.

DR. G. LOMBARD KELLY, vice dean of the University of Georgia School of Medicine, announces a gift of $18,000 from Mrs. John W. Herbert, of New York and Augusta, for the furnishing and equipment of the new wing of the University Hospital, as a memorial to her husband, the late John W. Herbert, and to two of their children, Mrs. Gertrude Herbert Dunn and John Oliver Herbert.

THE board of regents of the University of Wisconsin has accepted a grant of $57,500 from the Rockefeller Foundation to continue research on hormones. Funds for the research formerly came from the National Research Council out of funds provided by the Rockefeller Foundation. The funds permit Dr. F. L. Hisaw, professor of zoology, Dr. L. J. Cole, professor of genetics, and Dr. E. L. Sevringhaus, associate professor of medicine, to continue research on hormones or internal secretion glands which, it is thought, control the growth and actions of the human body.

A GRANT has been awarded by the committee of scientific research of the American Medical Association to Dr. W. Antopol, Dr. A. Schifrin and Dr. L. Tuchman, to aid in the continuation of experimental work on acetyl choline, especially in its relation to carbohydrate metabolism, which is being carried out in the chemical laboratories of the Mt. Sinai Hospital.

A COLLECTION of fifteen thousand specimens of wood was bequeathed to the Franklin Institute Museum by the late Henry Howson, of Philadelphia. Each piece is numbered with a steel die and bears the common as well as the botanical name.

THE National Committee for Great Britain and Ireland of the International Society for Microbiology has arranged to organize a congress in London in 1936, and an executive committee, with Professor J. C. G. Ledingham as president, Dr. R. St. John-Brooks as secretary and Dr. J. T. Duncan as treasurer, has been appointed to undertake the work of organization. The scientific business of the congress will be conducted in seven sections, which will meet daily during the meeting. The preliminary selection of programs for these sections will be entrusted to sectional sub-committees, which will report to the executive committee. Each sectional sub-committee will consist of local or London members, together with a recorder and secretary, and of corresponding members at home and abroad. Sections will be as follows: Section 1, bacteria in their morphological, cultural and physiological aspects; section 2, viruses, virus diseases, experimental tumor research and tissue culture; section 3, bacteria and fungi in relation to disease in man, animals and plants; section 4, economic bacteriology; soil, dairy research and industrial microbiology; section 5, medical, veterinary and agricultural zoology and parasitology; section 6, serology and immunochemistry, and section 7, microbiological chemistry.

THE annual meeting of the Association of British Zoologists was held by invitation of the Zoological Society in the society's rooms in Regent's Park on January 5. Professor W. A. F. Balfour-Browne presided. On this occasion the greater part of the time of the meeting was given to a discussion of the present outlook and aims of zoologists. In this discussion Professor D. M. S. Watson, Jodrell professor of zoology and comparative anatomy at the University College, London; Dr. J. Gray, fellow of King's College, Cambridge; Dr. E. S. Russell, director of the fisheries laboratory of the Ministry of Agriculture and Fisheries; Professor E. W. Macbride, professor of zoology, Imperial College of Science, and several other speakers took part.

UNDER the terms of the will of the late Virginia Purdy Bacon, of New York, the Smithsonian Institution some years since was bequeathed the sum of $50,000 to establish a traveling scholarship as a memorial to her husband, Walter Rathbone Bacon, for the study of the fauna of countries other than the United States. The amount available is the interest on the capital invested, which is about $3,000 a year, the incumbent to hold the scholarship not less than two years. Application for this scholarship, addressed to the secretary of the Smithsonian Institution, should be submitted not later than March 15. The application should contain a detailed plan for the proposed study, including a statement as to the faunal problems involved; the reasons why it should be undertaken; the benefits that are expected to accrue; the length of the time considered necessary for

the carrying out of the project; the estimated cost; and the scientific and physical qualifications of the applicant to undertake the project. The scholarship will be awarded for a term of two years. If at the expiration of the term it is desired to extend the time, the incumbent shall make application a sufficient time in advance, accompanied by a statement as to the necessity for such extension. All collections, photographs, records and equipment become the property of the institution. The incumbent shall not engage in work for remuneration or receive salary from other sources than the institution or its branches during the period of occupancy of the scholarship.

DR. L. O. HOWARD, formerly chief entomologist in charge of the Bureau of Entomology, U. S. Department of Agriculture, writes: "Government officials of Denmark paid last autumn a very good and well-deserved compliment to the work done in the United States in a certain branch of science. They invited Dr. Adam G. Böving, who for twenty-two years has been working in the U. S. National Museum and in the Bureau of Entomology of the Department of Agriculture, to cross the Atlantic and to give two short courses of lectures in Denmark. One of these courses, given at the Royal Museum of Zoology, of the University of Copenhagen, related to the classification of the larvae of the Coleoptera. This is a subject that in the last century was studied by the famous Danish workers, Schiödte and Meinert. Böving worked with Meinert and, fortunately for us, fate brought him to the United States and to Washington, a generation ago. These lectures were well attended by Danish students and workers and by some men from adjoining countries. The book by Böving and Craighead published in 1931, entitled 'Illustrated Synopsis of Larval Forms of Coleoptera' and which has been termed 'epoch-making' by certain Europeans, incited this course. The second course was delivered before the Royal Veterinary and Agricultural College and aroused much interest. The agricultural journals and the daily press paid much attention to these lectures. They related to applied entomology in the United States and especially described our organization and methods of work. Both courses were given in September."

IN SCIENCE for January 4 on page 24 the address for Dr. Gustav Zechel should have been University of Illinois instead of University of Chicago.

DISCUSSION

FILM-STRIP COPIES OF SCIENTIFIC PUBLICATIONS

ATTENTION was called some months ago[1] to the efforts being made to reorganize the production and distribution of scientific publications. Special emphasis was laid on the need of making published results of research more easily accessible to those who use them. It was pointed out that the apparatus and materials required for making film-strip copies of printed pages exist at present and all that is needed is to assemble them and systematically organize the service. A plan of such an organization was suggested, and a preliminary estimate given of the cost of the equipment required.

At the time the articles referred to above were written, the most highly developed machine for photographing pages of books upon moving picture film, of which I had learned, was one of German manufacture, for which the quoted price, in marks, corresponded to about $1,000.

Since then, through the courtesy of Dr. Robert C. Binkley, chairman of the Joint Committee on Materials for Research, I have had the privilege of reading the advance sheets of chapters IX and X of his revised "Manual of Methods of Reproducing Research Materials." These chapters give a comprehensive critical survey of the factors involved in applying film-copying processes to the reproduction of printed or other documents. The contributions of a large number of workers are reviewed and it is apparent that greater progress has been made than is generally realized. Some 12 film-copying cameras are described and their relative merits discussed. Of these, the camera invented and built by Mr. Lloyd B. Kennedy, of Warren, Ohio, is considered to be the most ingenious so far developed.

Of the several cameras at present on the market, the most widely used is the Leica. This camera, however, is designed particu'arly for the use of individuals who wish to make their own copies of documents. Its limited film capacity restricts the usefulness of this camera for the large scale production under which a highly organized library copying service would be called upon to operate.

On November 5 last Mr. Watson Davis, of Science Service, invited to a luncheon at the Cosmos Club of Washington about 15 persons known by him to be interested in the subject of film copying of documents. Among those present was Dr. R. H. Draeger, of the Medical Department of the U. S. Navy. Dr. Draeger

[1] SCIENCE, 80: 70–72, July 20, 1934; pages 184–5, August 24, 1934. See also address on the Berthelot Centenary, SCIENCE, 67: 497–99, May 18, 1928.

told of a camera he had built and exhibited film-roll copies of books made with it. He was led to build his camera by the desire to provide himself with a conveniently transportable film library of scientific publications, for consultation and study while on shipboard or at distant naval posts. The camera was completed shortly after he was detailed for a special course of study at the Naval Medical School of Washington. While here, the director of the school recognized the utility of the camera for copying rare volumes needed to complete files upon naval hygiene being assembled at the school. Upon his recommendation, Dr. Draeger was provided with facilities for constructing a second and more highly perfected camera, which is expected to be completed shortly.

As a result of this information communicated at the luncheon, Miss Claribel R. Barnett, librarian of the Department of Agriculture, who was also present, has arranged at my suggestion for Dr. Draeger to install his camera in her library and make experiments upon copying articles in bound volumes of journals, the loan of which in large numbers is requested by out-of-town governmental and private research institutions. This experimental service has been in operation for over two months and the most varied lot of articles have been copied and sent out.

The experiment has conclusively shown that this first stage of a film-copying service has been satisfactorily achieved. The only other improvements will be in the direction of increasing the automatic character of the apparatus. The next step is the designing and manufacture of film-strip magnifiers and projectors which will permit one to comfortably read the printed matter photographed on the film.

The many film projectors at present on the market have been designed for other purposes and can not be conveniently used for reading film strips without more or less modification. In general, they are equipped with holders for film rolls and not short strips. Furthermore, the lens focus is usually too long to permit the screen to be placed conveniently near the projector. This latter disadvantage can in some cases be overcome by projecting into a box provided with a mirror which reflects the image upon a ground glass.

In view of the need of reading equipment especially designed for film-strip copies of printed pages, attention has been turned first to constructing a simple magnifier. This consists of a short cylinder, to one end of which is fixed a holder to receive the film, beyond which is a ground glass, and to the other a lens capable of being focused to sharp definition of the image.

The film strip is inserted, the apparatus is held towards a source of light, the focus adjusted and the brightly illuminated text, which is magnified about 10 times, read without difficulty. A film-strip magnifier of this kind, provided with a satisfactory lens, a handle and a screen to shade the eye not used, has been constructed at a cost which would permit it to be sold for not more than five dollars.

Experiments with such a magnifier have shown that it can also be used for projecting the film copy upon an improvised screen or a reflecting mirror and ground glass mounted in a box. All that is required is an efficient source of light. This may conveniently be composed of a lamp, condenser lenses and a reflector mounted in a small metal box. A clamp bracket permits the light source and film magnifier to be held in line and directed towards any improvised screen or into a reading box provided with a mirror and ground glass. Of the three elements composing the apparatus, film strips may be read directly, using the magnifier alone, or by projection on an improvised screen with the aid of the magnifier and light source, or by projection on a ground glass by a combination of the three elements. As mentioned above, the film-strip magnifier with a lens suitable for reading the printed matter directly should cost not more than five dollars. Provided with a better lens, needed for projecting to a larger scale, the price would probably be within $10. The other two elements of the apparatus can no doubt be furnished for another $10.

In the previous communications upon the organization of a film-strip documentation service it was suggested that the film strips should be mounted in windows in filing cards and sent out in this form. The object of this was to facilitate filing and permit the title and reference to be typewritten on the card and thus the subject-matter identified without resorting to magnification or projection of the film copy. The high cost of equipping projectors with holding devices for cards as large as would be necessary makes this plan impractical. It is now suggested that the film strips be filed in envelopes with the necessary indications written on the outside. For reading, the film strips could then be removed and inserted in the ordinary sized slots provided on magnifiers and projectors.

In order to facilitate the identification of the subject-matter of a given film strip it is contemplated that the title page of the volume in which appears the article copied shall be photographed in the first frame of the strip, together with the abbreviation of the journal title, volume, page and year, copied from a legend sufficiently large to be read on the film strip with the naked eye. Thus the film copy will be so perfectly identified that the possibility of its being misplaced from its proper envelope need not be a cause of concern.

The film-copying service would at first consist simply of photographing the pages of articles in journals and sending the negative film strip to those desiring them. The cost would be very low. Acetate

film sells for $20 per 1,000 feet, and since two pages are photographed upon each 1½ inches of film there would be 16 pages per foot. Allowing for the title page, identification reference and a short blank space at each end, the film for a 10-page article would cost about 2 cents. The developing and labor would probably not amount to more than 8 cents per 10-page article, hence it is likely that the service could be rendered by a library, without loss, at 10 cents per article of 10 pages or less and 5 cents for each additional 10 pages. This, however, is only a preliminary estimate and may be subject to revision on the basis of experience gained during an experimental period of operation.

When one considers the complex and expensive organization required for keeping track of borrowed books, the wear and tear to which they are subjected, and the messenger or other service required to deliver them, the saving effected by reducing the number which would leave the library would certainly be an important item. It is even possible that film-copying service rendered free might be a saving over the present system of lending library books. It is therefore not unreasonable to expect that even at the low price mentioned, the adoption of film-copying by libraries would lead to a considerable economy of operation.

With this end in view and also in consideration of the great service that film copies may be expected to render research workers, Miss Barnett has arranged to have made, at the prices mentioned above, with the equipment of Dr. Draeger, film-strip copies of articles contained in publications on file in the library of the Department of Agriculture.

Those desiring to avail themselves of this service should send their orders to the "Biblio Film Service," care of Library, U. S. Department of Agriculture, Washington. It is expected that within a short time film-strip magnifying and projecting apparatus, such as described above, will be available.

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ORIGIN OF PETROLEUM

THE notes on this subject by J. M. Macfarlane and E. Berl recently published in SCIENCE are worthy of some comment. Macfarlane appears to favor the old theory of the decomposition of fish oil, or lime soaps of fish oil, by heat. Berl believes there is evidence that the source material of both coal and oil was "carbohydrates and carbohydrate-humic acids."

The writer has pointed out in two recent papers[1] that the older theories of petroleum origin were proposed almost entirely without consideration of the chemical character of petroleum and with little reference to or knowledge of the conditions of its geological occurrence. It was also pointed out that there is abundant factual evidence, of both chemical and geological nature, that petroleum has had a low temperature history, of the order of 100° F. There is also abundant evidence against the early, but still widely prevalent, idea that petroleum is nevertheless the result of heat decomposition of fatty oils or other organic material, these decompositions being assumed to take place at low temperatures by virtue of the great periods of time available, in the case of the older strata, for such change. The evidence is much too abundant to summarize adequately in this brief note.

Berl evidently accepts the evidence of low temperature history. It is a pity that theories of "distillation" and heat decomposition, set up years ago on the simple experiments of Warren and Storer (1867) and of Engler, which do much violence to the many chemical and geological facts that we now know, should continue to clutter up our scientific literature. Surely we owe it to youth, seeking to learn, to clear some of our scientific debris.

The chemical history of petroleum is still bristling with unsolved questions, but how to produce petroleum by cooking fish is not one of them.

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ARE FISHES THE PRINCIPAL SOURCE OF PETROLEUM?

DR. MACFARLANE'S recent communication in SCIENCE[1] calls to mind his theory that fishes are the principal source of material from which natural petroleum has been derived.[2] Even admitting that petroleum may have been derived from fish oil in the rocks by natural processes, he has failed to present convincing evidence of fish remains in sufficient quantity to account for the enormous quantities of petroleum in some formations, having attempted to account for the large quantities in other formations by assuming, without proof, migration from far distant sources, and ignored all other as likely sources. In his interesting book he assumed, for example, that fish remains are very abundant in the Green River oil shales. As I have elsewhere stated, such remains are confined almost entirely to a thin series of strata in a very small area of that thick, wide-spread formation.[3] Even in the limited region where the beautiful fish skeletons are

[1] *Bull. Am. Ass'n. Petr. Geol.*, 15: 611, 1931; *Jour. Inst. Petr. Technol.*, 20: 177, 1934.

[1] SCIENCE, November 23, 1934.
[2] Macfarlane, "Fishes the Source of Petroleum," The Macmillan Company, 1923.
[3] Henderson, *Proc. California Acad. Sci.*, 4th series, Vol. XV, pp. 269–278, 1926.

found they are not very abundant, and at most localities and horizons they are so scarce as to be wholly negligible. If the fish bones and teeth were destroyed by organic acids, that would have left the scales intact, as actually happened in the Mowry formation. Dr. Macfarlane perhaps also overestimated the abundance of fish remains in the latter formation. True, scattered scales are quite numerous in portions of the formation, but a few fishes would account for a great many of the scattered scales. The Pierre formation is yielding petroleum in quantity at many localities, yet fish bones, teeth and scales are very scarce throughout the formation at all the numerous localities I have examined, and there are no extensive fish beds from which one may safely assume that the oil has migrated. Many such examples may be enumerated, while places where fish remains are abundant in the neighborhood of oil fields have not been found over great areas occupied intermittently by oil fields. On the other hand, some of the oil-bearing formations contain vast quantities of remains of mollusks, diatoms, foraminifers and other organisms that may have stored in the rocks enormous quantities of carbonaceous material, which may be a source of petroleum. In addition, forms of algae and protozoa without durable parts that would be preserved in recognizable condition in the rocks, some of which represent groups that produce numerous generations per annum, may have deposited carbonaceous material equalling or exceeding the bulk of all other organisms. The protoplasm of all these organisms contain the elements entering into the composition of petroleum. I see no *a priori* reason why any or all of them may not have contributed toward the petroleum.

It is impossible to duplicate experimentally all the deep-seated natural conditions within the thick geological formations, such as heat, pressure, chemical associates and more particularly the time factor. Failure to produce petroleum experimentally from any organisms would not prove conclusively that it could not happen or has not happened under natural conditions during a very long lapse of time. Success in such experiments possibly would not conclusively demonstrate that the same thing has happened in nature. If petroleum is of organic origin, as is rather generally believed, experimentation and discussion, in order to command complete respect, must take into consideration all forms of animal and plant life, and especially the microscopic forms so abundant and almost universally distributed in both fresh and marine waters, all composed chiefly of the elements that enter into the composition of petroleum, each individual of many of the species containing a minute globule of oil.

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SCIENTIFIC BOOKS

PHYSICAL THOUGHT

The Development of Physical Thought. By LEONARD LOEB and ARTHUR S. ADAMS. John Wiley and Sons, New York.

ACCORDING to the preface, this book is the outcome of a course of lectures prepared and given at the University of California by the senior author. The notes of these lectures were used and revised by the junior author and are published as a joint production.

There is wide recognition of the difficulty of the graduate student and the younger physicist in coordinating his rather patchy knowledge and in getting a proper perspective of his science. The historical chronological development of a science is perhaps the natural one. Ideas grow. Even the mistakes and false starts are of value. The student who studies the development of ideas, including the errors, is sure to obtain a knowledge of the growth of his science which will be useful in his later specialization and teaching. A comprehensive knowledge of the growth of physical ideas is also particularly valuable to the teacher who is presenting the science to the beginning student as a cultural subject.

This book should be a help both to the student and to the teacher of physics, and the reviewer recommends it to their attention. The first chapter, which is headed "Historical," gives an interesting account of the thought and activities of the early Greeks and Romans and the limitations of their scientific methods. The importance of Aristotle and his great influence on thought through the Middle Ages is discussed and stressed. Also the authors point out the great influence of economic and political conditions on the growth of science.

The succeeding chapters take up successively the development of mechanics and dynamics, heat and the structure of matter, electricity and magnetism, light, and finally the electrical structure of matter and the new physics. The space given to each of these topics varies considerably, perhaps according to the special interest of the authors. The chapter on light might have been more extensive and clearer, particularly in the treatment of refraction and dispersion. The last chapter on the new physics is much the longest. This is not so desirable, as the young physicist is apt to know this field fairly well. A philosophical grasp of the growth of classical physics is more important for him and also more difficult.

Just because this is a book that may be largely used by the student of physics it is the duty of the reviewer to point out a few things that are not clear or that may be in error. The treatment of certain topics of the relativity theory is not clear and might be improved. The attempted explanation of the bending of a ray of light on passing the sun is rather confusing, and the student would have difficulty in grasping its meaning. The reviewer makes this statement with some hesitation, as he recognizes the difficulty of presenting relativity ideas in simple terms.

There appears sometimes to be confusion in the ideas of our younger physicists as to the development of certain concepts and results of the relativity theory. This confusion is somewhat evident in this text. At least there are some statements that are not clear. One might cite as a case in point the discussion of the dependence of mass on velocity. On page 164 the authors say: "Other theories based on the electrodynamics of Maxwell gave a change of mass with velocity when the velocity approached that of light, but the equations derived were not substantiated by the experiments of Bucherer." This is not a proper statement of fact. The Lorentz electron equation,

$$m = \frac{m_0}{\sqrt{1-\beta^2}}, \text{ where } \beta = \frac{v}{c},$$

was derived on the classical theory assuming a fixed ether. (See theory of electrons by Lorentz, Columbia University Lectures, 1906.) Historically, the matter is of some interest. Independently of the relativity theory not only was the above equation derived by Lorentz, but Bucherer himself derived a classical expression for the variation of mass, namely

$$m = \frac{m_0}{(1-\beta^2)^{1/3}}.$$

These were both deformable electrons postulated on a fixed ether. The experiments of Bucherer were undertaken to find out whether either of these expressions (or the earlier one of Abraham) was the correct one. His results indicated that the Lorentz equation represented the facts better than his own or Abraham's. One infers from the text, although the statement is not specifically made, that these experiments are a proof of the special relativity theory. However, the experiments of Bucherer could not and did not distinguish between the hypotheses assumed by Lorentz and Einstein. The writer of this review is a proponent of the relativity theory, but he would like to point out that *logically* the experiments of Bucherer are not a *unique* proof of the special relativity theory. They only prove the validity of the equation

$$m = \frac{m_0}{\sqrt{1-\beta^2}}.$$

Another unclear passage is the discussion of the residual advance of the perihelion of Mercury. The authors in discussing the above equation say: "For most earthly motions the relation is such that the mass does not vary appreciably with the velocity, but when v begins to be of same order of magnitude as c, it is seen $(1 - v^2/c^2)$ becomes less than 1 and the mass will increase. A case in point is the variation of the velocity of Mercury in its orbit, which becomes sufficiently great that the mass of the planet computed on the Newtonian basis is no longer able to account for its motion, hence the discrepancy in the calculation of the motion of the perihelion of Mercury mentioned above." This is not very clear, but it seems to mean that the advance of the perihelion of Mercury arises from a change of mass with velocity. This, however, is not the case.

In an elliptical orbit according to the Newtonian Law there is a relation

$$F/m = a$$

the acceleration for each point of the orbit. As the velocity changes in the orbit the mass will change also to a value m'. But according to the general relativity theory gravitational force and mass are proportional. The force F will change also to a value F' in such way that

$$F'/m' = a$$

the same acceleration as before. The ratio of the new force to the new mass will be the same as in Newtonian motion. The residual advance of the perihelion of Mercury arises from a modification of the space due to the sun's gravitational potential. The case is quite otherwise with the motion of an electron in an elliptical orbit about a central charge. Here the mass changes with the velocity, but the force does not. The advance of the perihelion in the case of the electron is described by the *special* relativity theory (or Lorentz equally well). The advance in the case of the planet Mercury is described by the *general* relativity theory.

The reviewer found the chapter headed "Historical" quite interesting. It should stimulate the student to further reading in the history of science.

It is desirable to point out one or two errors of date for future correction. Carthage was destroyed at the end of the Third Punic War 146 B.C. and not 201 B.C., as stated in the text. Also the authors state that "the fakir and simpleton" Cagliostro was put to death by the inquisition in 1750. Cagliostro was born in 1743 and died in prison in Rome in 1795. He can not properly be called a "simpleton." He was rather a very clever and unscrupulous charlatan.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

A GLASS ASSEMBLY FOR SEITZ BACTERIOLOGICAL FILTERS

BACTERIOLOGICAL filters are so made that the filtered liquid comes in contact with metal surfaces which usually contain copper or some other heavy metal. On one occasion a drop of water suspended from the stem of a well-washed and freshly sterilized Berkefeld filter contained enough copper to have a distinct blue color and give a flame test for copper. In view of the known static action of very small amounts of heavy metals for bacteria, even in protein media, it seems possible that contamination by copper or other metals may sometimes be sufficient to cause erratic variation in the growth of bacteria in synthetic media.[1] Such

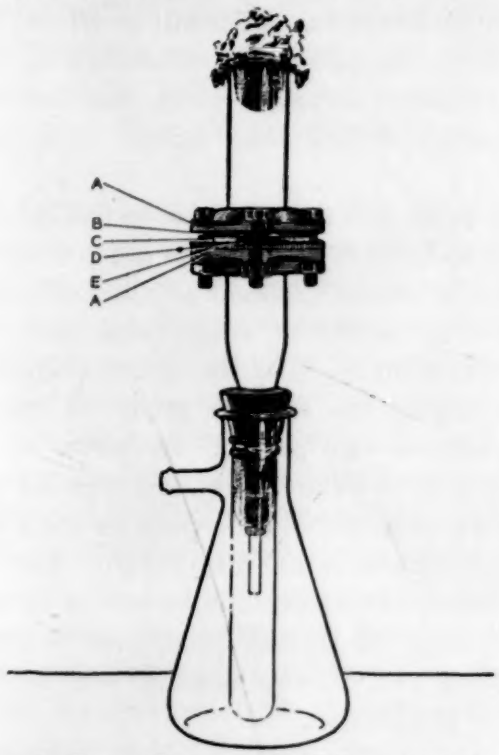

FIG. 1. A. Iron flanges. B. Filter paper. C. Seitz filter disc. D. Aluminum or platinum support. E. Asbestos interface gasket.

variation was in fact observed in this type of medium when the usual filters were used in sterilization, but not when a glass filter assembly was used.

In order to escape such an obvious possibility of contamination by heavy metals, a modified Seitz filter has been devised in which the liquid comes in contact with no metal surfaces other than aluminum or platinum. In addition to being easily constructed and allowing full vision of the material being filtered, this device has the advantage of being much less expensive than the usual filter of this type.

The heavy pyrex glass piping which has recently become available in a variety of sizes can be used to construct such a filter. For the filtration of 50 ml or less, two straight 4 in. lengths of 1 in. flanged piping with the metal flanges, bolts and asbestos interface gaskets to fit may be used. One of the straight lengths is cut in two, and to each half is sealed an appropriate funnel tube. If suction alone is required, only one of these need be used. The flanges and gaskets are then put in place, and between them is inserted a Seitz filter pad. This is supported on a sheet of aluminum foil, which may be 5/1000 in. thick or more, patterned after the gasket and perforated by about 50 pin-holes. The bolts and nuts are adjusted loosely, and after the usual preparation for filtration, the apparatus is sterilized in the autoclave. The bolts are tightened with a small wrench before the apparatus is used.

The filter pad requires support, since atmospheric pressure is sufficient to break through the wet pad. The aluminum support here described has given satisfactory service over a period of six months. There is no visible corrosion in the central part, and but slight corrosion at the exposed edges. Platinum gauze has been used, but no great advantage over aluminum has been discovered. The apparatus assembled for filtration by suction only is shown in Fig. 1.

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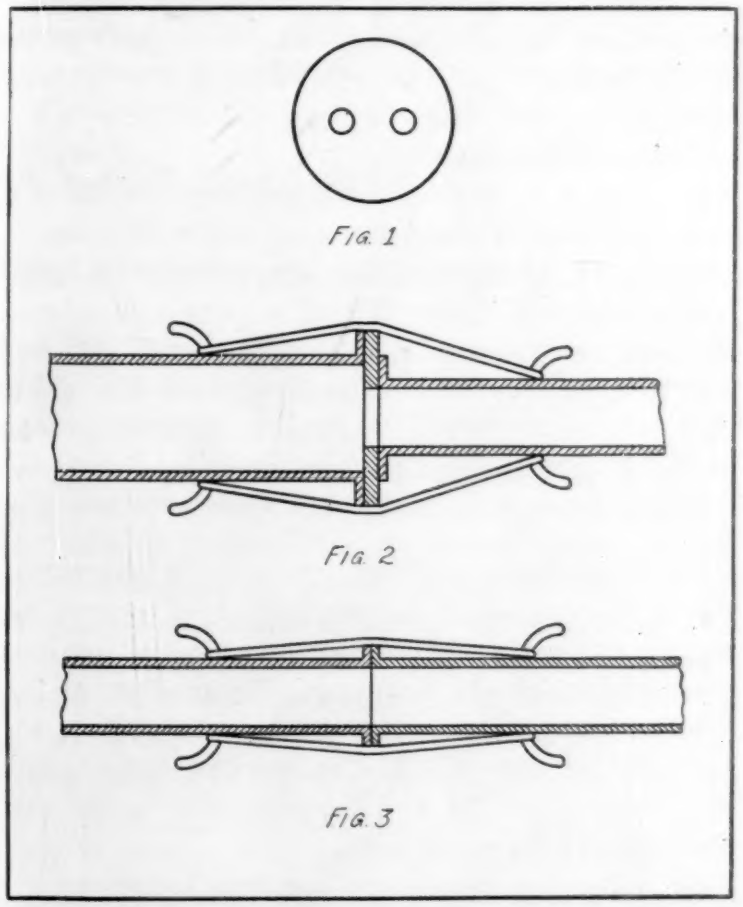
[1] E. O. Jordan and I. S. Falk, "Newer Knowledge of Bacteriology and Immunology," University of Chicago Press, 1928, p. 284.

A SIMPLE GLASS CONNECTION

AN easily made glass to glass connection which will serve satisfactorily in many glass apparatuses where



only small pressures or vacua are developed is shown in Plate 1.

Pieces of tubing, mouths of flasks, etc., are simply flared and given a flat lip, and then this lip is grounded in the usual manner by emery on a flat glass surface. The customary small glass arms for anchorage of rubber bands for holding pieces together can be easily fused to the main element.

The writer has built "T's," "L's," and straight lengths of tubing of varying lengths which are all surprisingly interchangeable. Figs. 1 and 2 give a simple method for reducing the diameter of a tube—*i.e.*, by the insertion of a ground glass disk with a hole in it. The ground glass disk idea is also satisfactory where it is desired to have two tubes connect with one vessel or another tube. Fig. 3 shows a straight connection involving the same size tubing.

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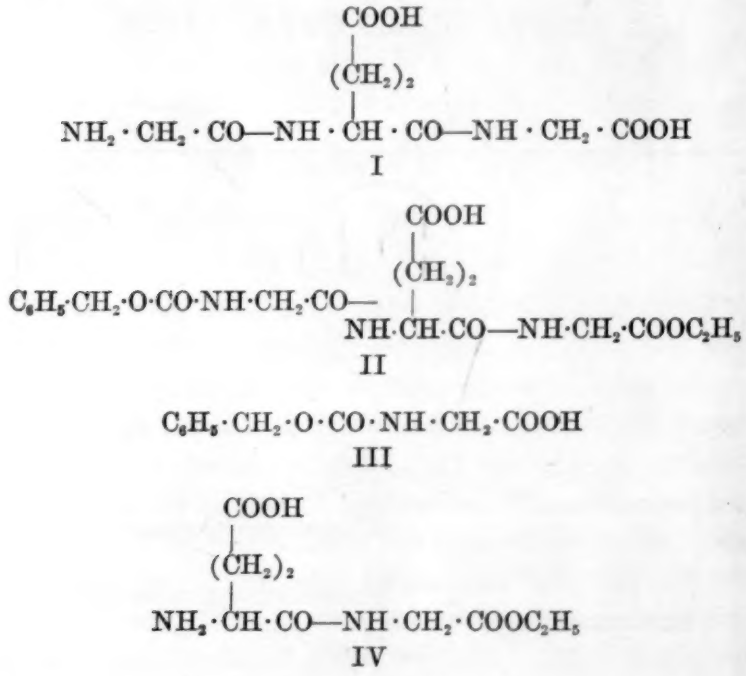
SPECIAL ARTICLES

A SYNTHETIC PEPTIDE AS SUBSTRATE FOR TRYPTIC PROTEINASE

LITTLE is known regarding the mechanism and the specificity of those enzymes which split true proteins —the proteinases. This is due to the fact that until recently it was not possible to obtain a proteinase substrate of known structure.

By means of the carbobenzoxy method[1] a peptide-like substrate for tryptic proteinase was synthesized. It is a derivative of the tripeptide glycyl glutamyl glycine (I). The amino group at one end of the molecule was blocked with the carbobenzoxy group, and the glycine carboxyl on the other end was esterified with ethyl alcohol (II). Thus the only free reactive group was the γ-carboxyl of the glutamic acid.



Substance II was split quite rapidly by pancreatin Merck as well as by a preparation of crystalline trypsin (tryptic proteinase) kindly placed at our disposal by Dr. John H. Northrop. The products of the splitting were carbobenzoxy glycine (III) and glutamyl glycine ester (IV). The latter product is rapidly transformed to a diketopiperazine under the conditions of the experiment.

This experiment shows that tryptic proteinase does not require for its action linkages of unknown nature, but is able to split ordinary peptide linkages if the rest of the molecule fulfils certain structural requirements. In II one of these requirements is the presence of the free γ-carboxyl, which combines with the tryptic proteinase and thus enables it to split the peptide.

It is probable that the other amino dicarboxylic and diamino carboxylic constituents of the proteins play a rôle similar to that of glutamic acid in combining with proteinases by means of their extra acid or basic groups. From the work of Gurin and Clarke[2] it is to be expected that the ε-amino group of lysine in a protein combines with pepsin. By means of the carbobenzoxy method we are preparing peptides of lysine and aspartic acid and shall report on their behavior towards proteinases in the near future. The theoretical significance of these results as well as the interesting experiments of Matsui,[3] Ishiyama[4] and Shibata[5] on the splitting of diketopiperazines will be discussed in a future publication.

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THE ELECTRICAL RESPONSE OF THE VESTIBULAR NERVE DURING ADEQUATE STIMULATION

A STRIKING characteristic of the vestibular nystagmus which is produced in virtually all vertebrate species by the angular retardation incident to the termination of a prolonged period of uniform bodily rotation (also by the acceleration incident to the onset of such a period of rotation) is that this response ordinarily persists for a considerable time—often 20 or 30 seconds—after the cessation of its

[1] M. Bergmann, SCIENCE, 79: 439, 1934.

[2] S. Gurin and H. T. Clarke, *Jour. Biol. Chem.*, 107: 395, 1934.

[3] J. Matsui, *Jour. Biochem.*, 17: 163, 253, 1933.

[4] T. Ishiyama, *Jour. Biochem.*, 17: 285, 1933.

[5] K. Shibata, *Acta Phytochimica*, 8: 173, 1934.

objective stimulus. In order to account for the discrepancy between the duration of this response and the duration of its stimulus, two general theories have been advanced. The *peripheral* theory holds that the post-stimulus persistence of vestibular nystagmus is dependent upon a continuation of the excitatory process (inertial movements of the endolymph, displacement of the cupula, or the like) which is initiated in the vestibular receptors by the objective stimulus. The *central* theory, on the other hand, holds that the post-stimulus persistence of this response is dependent upon the action of a neural mechanism located within the brain, which, once properly excited by a stimulus, however brief, continues to transmit effective impulses to the muscles involved in nystagmus until either this "after-discharge" mechanism becomes self-damped (perhaps through a gradual lengthening of the refractory phase of the constituent neurons) or until its activity is checked by the occurrence of an opposing stimulus.

In the hope of obtaining possibly more definitive evidence than has previously been advanced in support of either of these two rival hypotheses, the writer has undertaken a systematic comparison of the duration of objective stimulation of the vestibular receptors and the duration of the action currents thereby produced in the vestibular nerve. By means of a vacuum-tube amplifying apparatus developed by Dr. E. G. Wever and Dr. C. W. Bray, of Princeton University, for use in their investigation of the electrical phenomena of the auditory nerve, it has been found possible to make the passage of impulses along the vestibular nerve audible in a telephone receiver. The results thus far obtained from the study of the common painted terrapin (*Chrysemys picta*) indicate that, at least in this type of subject (selected because of particular accessibility of the vestibular nerve), the action currents probably never last for more than a fraction of a second after the cessation of objective stimulation. When the terrapin is accelerated on a manually operated turntable, there is audible in the receiver a distinct burst of discharge; and when the animal is retarded there is audible another similar burst of discharges, even though the maximum angular velocity attained be quite moderate. During prolonged rotation weaker discharges may be heard more or less continuously, due presumably to the more or less constant stimulation of the vestibular receptors through slight unavoidable variations in the speed of rotation. However, at the end of rotation, objective stimulation definitely ceases; and in no case has the passage of impulses been heard for more than approximately half a second after the subject has come to rest.

It is conceivable, of course, that effective impulses may continue to pass over the vestibular nerve (due to a continuation of the excitatory receptor process) for many seconds after the end of objective stimulation, without necessarily being detectable by the technique just described. However, if further investigation, with oscillographic recording and a more accurately controllable method of stimulation, confirms the results thus far obtained, it will be reasonably certain that the post-stimulus persistence of vestibular nystagmus is due, not to a concomitant persistence of receptor activity, but rather to sustained after-discharges from a neural mechanism located within the brain.

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THE SPECIAL REACTIVITY OF PEPTIDES

IT is one purpose of the present note to point out that, whereas current theory seems to assume that most of the obvious transformations of amino-acids occur when these are present as such, some of these reactions may really take place much more readily as transformations of peptides.

For some time the writer has been interpreting the decomposition[1] of cysteine and cystine derivatives by alkalies as a reaction in which preliminary enolization allowed expulsion of the sulfur (with whatever might be attached to it) as a negative group. Theory and recorded fact seemed to agree as to the modifications of the amino- and carboxy- groups necessary to produce greatly increased reactivity. Peptides (and analogous compounds) were conspicuously more reactive than simple amino-acids, though less reactive (for reasons which will be explained elsewhere) than cyclic derivatives, such as diketopiperazines and hydantoins.

From the beginning it was assumed[1,2] that this type of reaction would be reversible. It has now been found possible[3] to add p-tolylmercaptan to α-acetylaminoacrylic acid to form S-p-tolyl-N-acetylcysteine, and this is now leading to a new cystine synthesis which will perhaps have a definite relation to the natural synthesis of cystine. The important point in the present connection is, however, that it is decidedly easier to add[3] mercaptans to benzoyl-dehydrophenylalanyl-glycine ester, which may be considered as a model of a dehydro-tripeptide. The addition as well as the elimination of sulfur derivatives thus occurs according to the principles already advanced. That is, it occurs more readily in a peptide which is at least a tripeptide, and in which the active portion of the molecule is not in a terminal position.

[1] B. H. Nicolet, *Jour. Am. Chem. Soc.*, 53: 3066, 1931.
[2] B. H. Nicolet, *Jour. Biol. Chem.*, 95: 389, 1932.
[3] Unpublished results.

Thus two reactions, the decomposition and the formation of cysteine and cystine derivatives, occur more readily in the case of peptides. To these may be added a longer known third reaction fulfilling the same conditions—the racemization of peptides[4,5] by alkali. A further extension of the principle will now be considered.

Is "Oxidative Deamination" a β-Oxidation?

Oxidative deamination[6] of amino-acids to α-keto acids under biological conditions is an accepted concept. So is, to a considerable degree, the intermediate formation of a "dehydro"-amino-acid, which is, however, formulated as $RCH:C(NH_2)CO_2H$ (A) or as $RCH_2C(:NH)CO_2H$ (B), according to the tastes of the particular author concerned. The two types are tautomers. Type B is almost surely involved in the hydrolysis; but whether A or B is originally formed in the oxidation is a matter of much interest.

Some theoretical aspects of a reaction leading to the initial formation of Type A will now be discussed. In the first place, such a reaction could conveniently be regarded as a special case of β-oxidation; and such a view-point allows the drawing of certain conclusions which, it seems to the writer, may eventually allow a decision to be reached.

It is clear that the "oxidation" of hydrogen, even when it involves merely the transfer to an acceptor other than oxygen, involves the approach of the hydrogen atoms concerned to a more positive character. But the hydrogen of least positive character, and therefore that most subject to oxidative attack, is located on the β-carbon atom, due to the effect of the carboxyl group. This effect, while in the case of a free amino-acid relatively feeble, is still very real; and it should favor the formation of a dehydrogenation product of Type A.

But two types of modification of the amino-acid structure should favor such β-activation quite strongly. These are just those modifications which have been reported as affecting so strikingly the reactivity of the sulfur-carbon bond in cystine derivatives; and excellent examples would be, peptide formation on both the amino- and carboxy- groups. In other words, *if* oxidative deamination is essentially a β-oxidation, and occurs through a derivative of Type A, it can be predicted that it will occur most readily in substances which have at least the complexity of tripeptides, and that in these the middle member of the tripeptide chain will be the most susceptible to dehydrogenation.

So far as the writer knows, no clear-cut distinction on this basis has been made as yet, either theoretically or experimentally. A variety of "model" experiments in deamination have recently been reported in which charcoal, with or without air, or substances of quinoid type, have been used to induce the reaction. These experiments, which can not here be cited in detail, show glycine to be attacked with exceptional ease. This is certainly not β-oxidation; but neither is it clear that the type of deamination used in the body is here represented. On the other hand, Krebs[7] has managed to demonstrate oxidative deamination of the usually assumed type most convincingly, since he has also isolated derivatives of the expected α-keto acids; but the kidney tissue with which he induced the reaction contained such a complex of proteolytic enzymes that it has not yet been possible to decide whether or not peptides are attacked more rapidly than the simple amino-acids. It is perhaps a modification of his procedure which will settle the question.

The main idea of this portion of the present discussion is definitely this: In so far as oxidative deamination under more or less biological conditions attacks peptides more rapidly than simple amino-acids, and the intermediate members of peptide chains rather than the terminal members, in just that measure it will appear probable that the first stage of oxidative deamination is a β-oxidation, leading to an initial product of Type A.

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BUREAU OF DAIRY INDUSTRY
BELTSVILLE, MARYLAND

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